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Estimating Returns to Scale with Large Imperfect Panels

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and
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Do policies that promote "bigness" in manufacturing plants also promote greater productivity? Does correlation between size and profitability constitute a case for anti-trust activity?

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This paper — a product of the Trade Policy Division, Country Economics Department — is part of a PRE research project on Industrial Competition, Productive Efficiency, and their Relation to Trade Regimes (RPO 674-46). Copies are available free from the World Bank, 1818 H Street NW, Washington DC 20433. Please contact Sheila Fallon, room N10-021, extension 37947 (47 pages).

Tybout and Westbrook provide systematic panel-based econometric estimates of plant-level returns to scale for various 3-digit and 4-digit manufacturing industries, using panel data for Chilean plants. Their findings shed light on several issues of interest to policymakers.

First, do policies that promote "bigness" in manufacturing plants also promote greater productivity? As plants grow, do they become more efficient?

They find that although several 4-digit sectors show increasing returns, general expansion of the manufacturing sector cannot be expected to yield strong economies of scale at the plant level. Taking their "best" estimates at face value, the returns to scale in manufacturing are scattered across the range of 0.8 to 1.2 at the 3-digit level and 0.7 to 1.6 at the 4-digit level. None of the 3-digit returns-to-scale (RTS) estimates is significantly different from unity, and only two of the 4-digit estimates are.

Second, it appears that plants that are inherently more efficient tend to grow larger, as Demsetz and others have argued. This inference is based on a comparison of RTS estimates that control for unobservable efficiency effects with estimates that do not. It implies, among other things, that positive correlations between size and profitability need not constitute a case for antitrust activity.

A corollary to this finding is that most RTS estimates based on cross-sectional data tend to overstate plant-level returns to scale.

As a byproduct, their analysis appears to have reopened the possibility of using Stigler's survival test to gauge the importance of returns to scale. But unlike earlier applications of this test based directly on the distribution of plant size, their results suggest using Probit estimates of the elasticity of failure probabilities with respect to plant size as crude proxies for RTS.

Estimating Returns to Scale with Large Imperfect Panels

by
James R. Tybout
and
M. Daniel Westbrook*

Table of Contents

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1. INTRODUCTION

Domestic markets for industrial products are often small in developing countries. Accordingly, scale economies in the manufacturing sector can critically influence market structures, growth prospects, and trading patterns. While the potential importance of these effects has long been recognized by students of development, there has been very little convincing research on their empirical significance.[1] This study exploits plant-level panel data from Chile to provide direct new evidence.

The existing empirical literature is cloudy partly because data in developing countries are scarce. But more importantly, regardless of country, there are serious problems with the methodologies that have been used to document returns to scale (RTS). At least four approaches are in the literature. The first amounts to asking managers what size they would need to be to reach maximum efficiency. The problem with this methodology is that managers have some sense for the profitability of plants at alternative sizes, but they are likely to have trouble isolating the effects of technology from other factors. For example, when market power is exercised by large firms, managers may confuse their relative profitability with scale efficiency. Also, even if big plants do have lower unit costs, it may be because plants with superior management and/or market niches tend to grow large, while others shrivel and die. Hereafter this phenomenon will be referred to as the "Demsetz effect".[2]

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[1] For example, Bhagwati (1988) laments that "...although the arguments for the success of the export promotion strategy based on economies of scale and X-efficiency are plausible, empirical support for them is not available." Similarly, Rodrik (1988) notes that "...there is practically no direct evidence on the importance of scale economics in specific industrial sectors of the developing countries." Finally, Berry (1990) concludes that "neither the evidence on the relation of size to unit costs or profits nor the implications of survival analysis suggest a prevalence of economies of scale, scope, or size in LDCs. But neither do these data contradict such a possibility."

[2] Demsetz (1973) argued that this evolutionary process explains the correlation between size and profitability typically found in industrialized countries. Jovanovic (1982) has formalized the argument in a dynamic learning model where firms discover their efficiency through market experience, and eventually expand or exit.

The second approach is to ask engineers how big plants should be in order to be efficient. Berry (1990) notes that this type of analysis, in addition to being very costly, has its own biases. Engineers typically hold the basic technology fixed while varying output levels, so alternative technologies that are efficient at a small scale are ignored, and returns to scale tend to be overstated. Also, engineers tend to ignore non-production costs (e.g., management and distribution) that may rise more than proportionately with plant size.

Stigler (1958) advocated a third approach. He argued that industries exhibiting a wide range of plant sizes in perpetuity must have flat long run average cost curves. The obvious problem with Stigler's "survivor test" is that it presumes perfect competition, long run equilibrium, and no uncertainty. As Jovanovic (1982) and Jovanovic and Lach (1989) have shown, plants of varying efficiency can coexist indefinitely if they are learning about the market and their own technology. Product market imperfections confound matters further, as in Pakes and McGuire (1991).

Finally, econometric techniques can be used to estimate cost functions or production functions that allow the investigator to infer the relation between size and efficiency. One problem with this approach is that it requires data on plants with varying degrees of scale efficiency. If Stigler's survivor effect is operative, competition will omit inefficient plants from the sample and prevent the cross-sectional identification of returns to scale.[3] On the other hand, among industries characterized by "dominant-fringe" market structures, the sheer number of fringe firms may render the influence of the major producers negligible in standard estimators. Econometric studies based on cross-sectional variation are also likely to confuse Demsetz effects with scale effects, and to suffer from significant measurement error bias. The latter problem is especially acute for capital stocks and factor prices.

Despite the drawbacks of the econometric approach, several considerations lead us to

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[3] More subtle selectivity problems can bias studies based on panel data, as will be seen later.

use it in this study. First, we have access to a large panel data set that allows us to deal with Demsetz effects and measurement errors.[4] As will be seen, both of these phenomena turn out to be important. Second, unlike engineering studies and attitudinal surveys, panel-based econometric estimates infer RTS from the *observed* temporal variation in inputs and outputs. Hence they come closer to describing the *realized* scale effects that accompanied demand shifts and policy changes during the sample period.

The paper is organized as follows. Section 2 presents our assumptions regarding technology and behavior. Then Section 3 discusses alternative estimators that deal with different aspects of the econometric problems we face. Finally, applications of the alternative estimators to various 3-digit and 4-digit industries are reported in Section 4, and an attempt is made to determine which RTS estimates are the most reliable.

2. TECHNOLOGY AND BEHAVIOR

A. Technology

Our interest is in estimating plant-level RTS, controlling for other determinants of the relationship between inputs and outputs. To this end, we begin with a simple Cobb-Douglas representation of technology for a particular industry:

$$Y_{it} = \alpha L_{it} + \beta K^{*}_{it} + e_{it} . \tag{2.1}$$

Here $i = 1, \cdots, N$ is the firm subscript, $t = 1, \cdots, T$ is the time-period subscript, and the industry subscript is suppressed. Y is the logarithm of real value added, L is the logarithm of labor (measured in efficiency units), K^{*} is the logarithm of the true capital stock, and e_{it} is an error

[4] Our data cover virtually all Chilean manufacturing plants with at least ten workers over the time period 1979 - 1986. They were supplied to the World Bank by the Chilean government in connection with the research project "Industrial Competition, Productive Efficiency, and Their Relation to Trade Regimes," (RPO-674-46).

term.[5]

The error term is assumed to have three components that are unobservable to the econometrician:

$$(2.2) \qquad \epsilon_{it} = \mu_i + \tau_t + \xi_{it}.$$

The first component, μ_i, is a plant-specific effect that reflects heterogeneous technologies and management skills. The second component, τ_t, is a time effect that is common to all plants. It relects RTS at the industry level, general changes in capacity utilization, and technological innovation. Both μ_i and τ_t may be correlated with the exogenous variables. Remaining noise is represented by ξ_{it}, which is assumed to be identically independently distributed across plants and time and uncorrelated with the exogenous variables.

B. Behavior

To characterize producer behavior we adopt the perspective of Olley and Pakes (1990) and Pakes and Ericson (1988). Given current capital stocks, managers are presumed to maximize expected future profits by deciding whether to operate in the coming period, and if so, what investment and employment levels to choose. Because period t profits depend on μ_i and τ_t, and because managers are likely to have information on both of these error components, investment and employment levels are generally correlated with the disturbance ϵ_{it}. More precisely, under reasonable assumptions, the cross-sectional correlation between plants' productivity and their capital stocks is positive, so ordinary least squares estimates of equation

[5] Although this functional form is restrictive, it can be made more flexible by allowing the coefficients to vary across groups of plants. In particular, the coefficients can be indexed by the size range of firms being examined, thus providing a basis for testing whether measured returns to scale depend upon plant size [*cf.* Mellor (1975) and Griliches and Ringstad (1971)]. Our experiments along these lines (available on request) revealed no clear tendency for RTS to rise or fall with plant size, so we do not pursue this issue further herein.

(2.1) tend to overstate returns to scale.[6] This is the Demsetz effect, and any consistent estimator of production technologies must control for its presence. Factor demands are uncorrelated with ξ_{it} so long as its realizations are unanticipated by managers.

3. ESTIMATORS

A. Dealing With Plant and Time Effects

The bias due to Demsetz effects can be eliminated if the error components μ_i and τ_t can be removed from the production function disturbance; in this subsection we review the standard ways of doing so. For the time being it is convenient to assume that all explanatory variables are measured without error; this assumption will be relaxed in Section B.

The Within Estimator

Perhaps the most common way to sweep out the plant effects, μ_i, is known as "within" estimation. It amounts to including plant-specific dummy variables in the regression, which is equivalent to performing OLS on variables expressed in terms of deviations from their plant-specific means. That is, any variable x_{it} appearing in the regression is replaced by $\tilde{x}_{it}$:

$$\tilde{x}_{it} = x_{it} - (1/T)\sum_{t=1}^{T} x_{it}, \quad i = 1, \ldots n \tag{3.1}$$

The within estimator identifies structural coefficients by exploiting the temporal variation in the data. The time effects τ_t may remain a source of bias but they can be swept out by including annual time dummies in the model or by further transforming all variables $\tilde{x}_{it}$ to be

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[6] Formal representations of the correlation between market share and productive efficiency may be found in the industrial evolution models of Pakes and Ericson (1988), Pakes and McGuire (1991), and Jovanovic (1982). Of course, it is easy to establish this correlation in static frameworks [*cf.* Zellner, Kmenta, and Dreze (1966), Mundlak (1978), and Chamberlain (1984)].

deviations from their year-specific means. Elimination of time effects also serves to control for sector-wide measurement errors in ouptut growth due, for example, to inappropriate price deflators.

The Difference Estimators

An alternative way to sweep out plant effects is to difference the data. The j^{th} "difference estimator" amounts to OLS on variables transformed as:

(3.2) $$d^j x_{it} = x_{it} - x_{it-j};$$

where d^j denotes the difference operator. If there are T time periods in the panel, any j value between 1 and T-1 may be chosen.

Like the within estimator, this technique permits consistent estimation of the structural coefficients when plant effects are correlated with included explanatory variables. Time dummies can be used to control for variation through time that is common to all plants, as before. However, unlike the within transformation, the difference transformation yields transformed disturbances that involve only ξ_{it} and ξ_{it-j}, rather than than a weighted average of all years' disturbances. This feature of difference estimators affords more flexibility than the within estimator when treating measurement error or simultaneity problems. For this reason we base most of the remaining analysis on difference estimators.

B. Dealing with Measurement Error

The Problem

Thus far we have ignored the possibility of measurement error in observed capital stocks. If this assumption is unwarranted, none of the estimators described above is consistent. We now discuss the types of bias that this problem is likely to introduce and estimation techniques that eliminate them. Throughout, we assume that the measurement error plagues only the econometrician: plant managers are presumed to know K°_{it} when they choose factor stocks.

Suppose the capital stock observable to the econometrician may be written as the "true" stock, plus noise:

$$(3.3) \qquad K_{it} = K^{\circ}_{it} + v_{it},$$

where $E[v_{it}] = 0$, $\mathrm{var}[v_{it}] = \sigma_v^2$, $r_j = \mathrm{corr}(v_{it}, v_{it-j})$, and v_{it} is uncorrelated with μ_i, τ_t, and e_{it}. The j^{th} difference estimator (a_j, b_j) for (α, β) emerges from OLS estimation of:

$$(3.4) \qquad d^jY_{it} = \alpha(d^jL_{it}) + \beta(d^jK_{it}) + d^j(e_{it} - \beta v_{it}).$$

Generalizing Griliches and Hausman (1986), it can be shown that the associated RTS estimator $(a_j + b_j)$ has asymptotic bias ($n \to \infty$, T fixed):

$$(3.5) \qquad \operatorname*{plim}_{n \to \infty} [(a_j + b_j) - (\alpha + \beta)] = 2(\gamma - 1)(1 - r_j)\beta\sigma_v^2 / \mathrm{var}(d^jK_p) .$$

Here γ is the population regression coefficient when d^jL is projected on d^jK and time dummies, and $\mathrm{var}(d^jK_p)$ is the residual variation in the projection of d^jK on d^jL and time dummies. Notice that the bias is negative so long as $\gamma < 1$, and its absolute magnitude depends directly on the noise-to-signal ratio, $\sigma_v^2 / \mathrm{var}(d^jK_p)$.

As Griliches and Hausman (1986) note, noise-to-signal ratios depend on the relative magnitudes of serial correlation in K_p and v, which in turn depend on j. If K_p is covariance stationary, then $\mathrm{var}(d^jK_p) = 2[\mathrm{var}(K^{\circ}_p)(1-\rho_j) + \sigma_v^2(1-r_j)]$, where $\rho_j = \mathrm{corr}(K^{\circ}_{p\,it}, K^{\circ}_{p\,it-j})$. By equation (3.5), the smaller $(1-r_j)/(1-\rho_j)$ is, the smaller the RTS bias is. High r_j values are associated with low bias, *ceteris paribus*, because persistent measurement error is eliminated by differencing the data. Small ρ_j values are also associated with low bias because they increase the variance of d^jK_p, given var(K°_p). More generally, the asymptotic bias declines beyond some difference length if three plausible conditions are met: r_j reaches a lower bound at this difference length, var(d^jK_p) grows monotonically with j, and the auxiliary regression of labor on capital yields a coefficient $\gamma < 1$.

Correcting for Measurement Error

Going to longer difference estimators may well *reduce* measurement error bias, but it is unlikely to eliminate it entirely. Moreover, reliance only on the longest differences for parameter estimates means ignoring sample information. Measurement error bias can be eliminated without eliminating sample information by adopting the Generalized Method of Moments (GMM) estimator discussed in White (1982), Griliches and Hausman (1986), and Arellano and Bond (1988). This estimator has the added advantage of correcting for a general form of heteroskedasticity. A brief exposition of the GMM technique is provided in Appendix I.

The GMM estimator uses instruments to deal with measurement error, and the set of valid instrumental variables depends upon the process that the errors follow. If they are serially uncorrelated or follow a low-order MA process, then leads and lags of capital are available as instruments [*cf.* Griliches and Hausman (1986)]. However, for several reasons, capital stocks constructed using the perpetual inventory method are likely to reflect measurement errors correlated across long periods. First, measurement error in year t investment is spread to all future years in which the acquired assets are not fully depreciated. Second, the flow of services generated by a unit of capital may not smoothly decay at the assumed depreciation rate. More likely, the flow of services depends upon the vintage of the capital, as Pakes and Griliches (1984) find in their analysis of U.S. manufacturing.

Given the above observations, we consider three variables to be reasonable instruments for net capital in a given difference equation: the change in employment level between the inital and final periods of the difference, the change in net purchases of machinery and equipment between the initial and final periods of the difference, and the change in real wages between the initial and final periods. Each proposed instrument requires some justification. First, so long as managers don't anticipate ξ_{it} when choosing employment levels, employment is orthogonal to the current period disturbance. Second, if the measurement error in capital stocks comes from longer term items (land and buildings), machinery and equipment will be correlated with growth in the flow of capital services, but uncorrelated with the measurement error v. Finally, unless real wages are completely unpredictable they will be correlated with expected profits, and they should thus be correlated with true capital stocks.

Each of these arguments is subject to criticism, of course, so the results will be compared with those from un-instrumented difference estimators to check whether the coefficients move in the expected direction. If simple difference estimators are used, the biases due to measurement error are:

$$(3.6) \qquad \operatorname*{plim}_{n\infty} (a_j - \alpha) = [2(1-r_j)\sigma_v^2 / \mathrm{var}(d^j K_p)]\, \gamma\beta \qquad j = 1, \cdots, T-1$$

$$(3.7) \qquad \operatorname*{plim}_{n\infty} (b_j - \beta) = -[2(1-r_j)\sigma_v^2 / \mathrm{var}(d^j K_p)]\, \beta \qquad j = 1, \cdots, T-1$$

Hence instruments that eliminate measurement error bias should typically reduce the estimated α value, increase the estimated β value, and (by equation 3.5), increase estimated returns to scale.

C. Selectivity Bias

<u>The Problem</u>

It is well known that young plants tend to be small and to have relatively high failure rates. It is also true that among these plants, the least efficient ones fail more frequently.[7] So if plants that are not observed in all sample years are left out of the analysis altogether, the estimated change in input per unit change in output may be biased. For example, if the less efficient plants require relatively large increments to inputs per unit change in output, their omission would likely lead to RTS estimates that were too high. On the other hand, if the inefficient plants are those that are in the increasing-returns range, then selectivity bias might cause RTS estimates to be too low [*cf.* Pakes and Olley (1990)].

Estimators that Correct for Selectivity Bias

To examine the nature and importance of selectivity bias, we proceed in two stages. First, we apply the estimators introduced above to the subset of plants that is observed for the entire sample period. This "balanced" subsample is useful as a reference case because most studies of returns to scale deal only with such plants. We then add plants that are missing for some portion of the sample period, but that can be observed for some of the difference equations. This brings in plants that enter the sample and stay in for at least a year as well as plants that exit after the terminal year of the j^{th} difference equation.

Second, we make a selectivity correction for plants that are observed in the initial year of a difference equation but not in the final year. This corrects for the bias induced by systematically under-representing failing plants in the sample. The selectivity correction is made using a Heckman (1979) two-stage estimator, which amounts to estimating a Probit model that forecasts whether a plant present in year t-j is still present in year t, then using the resultant parameter estimates to construct a Mills ratio that is added to the set of explanatory variables in the j^{th} difference equation. The Probit equation that we use expresses the probability of survival as a function of the size of the plant (measured by number of workers in year t-j) and a dummy variable that indicates whether the plant is a new entrant in year t.

[7] For evidence that this is the case in our Chilean panel, see Liu (1990b).

The Probit model coefficients are allowed to vary across time so that the estimated probabilities can respond to changing economic conditions in a general way.

4. APPLYING THE ESTIMATORS TO CHILEAN DATA

A. Overview

Research Strategy

In this section we report on the application of various estimators to Chilean panel data and interpret the results. First, in part 4B, we look for evidence of the various specification problems discussed in Section 3. Comparisons of OLS and within estimators provide evidence on the importance of Demsetz effects; comparisons of long and short difference estimators (*inter alia*) provide evidence on the importance of measurement error; comparisons of balanced panel results with those based on extended samples, with and without Mills ratio corrections, shed light on the importance of selectivity bias. Then, in part 4C, we present results obtained with GMM estimators that are robust to the specification problems uncovered.

Data

The data we use cover virtually all Chilean manufacturing firms with at least 10 workers observed at least once during the period 1979 - 1986. Outputs are deflated using sector-specific output price deflators, intermediate goods are deflated using price indices constructed from sectoral output prices using the 1977 Chilean input-output table, and energy usage is measured using a plant-level Laspeyres quantity index based on physical volumes and values reported. Capital stocks are imputed by applying the perpetual inventory method to deflated investment figures for each of four capital goods categories.[8] A more detailed

[8] Base-year capital stocks are taken from 1980 financial statements. In 1979 firms were instructed to revalue their capital stocks according to market worth (the "retacacion tecnica"), so these statements should roughly reflect replacement costs.

description of the data may be found in Liu (1990a).

B. Evidence on Specification Problems

Demsetz Effects

As discussed earlier, so long as efficient plants grow more rapidly and survive longer than inefficient plants, ordinary least squares is likely to overstate returns to scale. This bias can be eliminated by sweeping plant-specific efficiency effects out of the disturbance term with either a *within* or a *difference* estimator, so comparisons of OLS results with either of these alternatives should suggest whether a bias is present. To this end, Table 1 reports OLS and *within* estimates for the various 3-digit industries.[9] Clearly the OLS estimates indicate that most industries exhibit increasing returns to scale, but the within estimates show returns to scale less than unity. Accordingly, we conclude that Demsetz effects are potentially significant, and hereafter we work only with *within* or *difference* estimators.

Capital Stock Measurement Error

If correlation between plant effects and factor stocks were the only problem with OLS estimators, the *within* estimator would be consistent. However, the *within* estimates of RTS reported in Table 1 are too low to be plausible, and other patterns in the data suggest that capital stock measurement error is part of the explanation. Specifically, recall from Section 3B that when certain conditions are satisfied, measurement error biases RTS estimates downward by an amount that delines with the length of the difference estimator used. These conditions are: (1) the serial in correlation of measurement errors, corr(v_{it},v_{it-j}), reaches a lower bound beyond some j, (2) variances in differenced capital stocks grow monotonically

[9] Though not reported, time dummies are included in all regressions. Estimates in this table are based only on plants that report data in all years; industries with less than 20 such plants are not analyzed.

with the difference length, and (3) the auxillary regression of labor on capital yields a coefficient $\gamma < 1$. Although the first condition cannot be directly addressed, Panels B and C in Table 2 suggests that the second and third conditions do hold. Moreover, Table 3 shows that, as predicted, short difference estimators typically yield returns to scale estimates substantially lower than long (fifth, sixth, and seventh) difference estimators, though this pattern is not evident for some industries, and in several industries it is not monotonic. We conclude that in many industries measurement error is a non-trivial problem.

Selectivity Bias

Selectivity bias may also partly account for the low *within* and *difference* estimates of RTS reported above. Recall from Section 3C that this bias may occur on two levels. First, if a balanced panel is used (as in Tables 1, 2, and 3), plants that do not appear in all sample years are left out of the analysis altogether. Second, even if an extended panel is used, plants that drop out of the sample before the final year of a particular difference equation will be left out of that equation. To gauge the first bias, we compare simple difference estimates based on balanced data with those based on all available observations for each of the equations. Table 4 presents the simple difference estimators for the extended samples, which display quite large increases in degrees of freedom. Their relation to the Table 3 estimates is summarized in Table 5, which shows the proportional increase in estimated RTS based on first through seventh difference equations. The change in estimated RTS is substantial for a number of industries, confirming the Olley and Pakes (1990) finding that omission of entering and exiting plants can lead to significant biases in technology estimates. It is clear that entering and exiting plants differ from incumbents not only in terms of their mean productivity levels (plant effects), but in terms of their RTS.[10] However, the manner in which they differ varies across industries.

[10] For further analysis of the nature of this difference, see Liu (1990b).

The dominant pattern seems to be that moving from the balanced to the extended sample increases estimated RTS among the shorter differences. The lack of response among longer differences is at least partly explained by the fact that disparties in sample coverage (balanced vs. extended) decline with length of the difference, disappearing entirely for the longest (seventh) difference.

The results based on extended panels may themselves be subject to bias if plants present in year t-j but not in year t differ systematically from those that surive the period. To investigate this effect we use Heckman's (1979) two-step procedure. First, to predict exit patterns between years t-j and t, we fit the following Probit equation:

$$(4.1) \qquad S_{it} = \alpha + \beta_{80} D_{80.i} + \beta_{81} D_{81.i} + \beta_{TL} TL_{it-j} + \varphi_{it},$$

where $S_{it} > 0$ indicates that the i^{th} plant exited between periods t-j and t, $D_{80.i}$ and $D_{81.i}$ are dummies that indicate whether the plant was a new entrant in 1980 or 1981, and TL_{it-j} is the log of the total labor force of the i^{th} plant in year t-j, which serves as as a proxy for firm size.[11] A different Probit is fit in cross section for each of the T-j years associated with the j^{th} difference estimator; the dummies are dropped in equations where they are irrelevant or an insufficient number of entering plants is observed.[12]

Table 6 reports results for the probabilities of survival across the years spanned by the

[11] We also estimated Probit equations that express the probability of survival as a function of the size of the plant and of its type of legal organization (proprietorship, partnership, corporation, or other). Business type did little to explain survival and the business type dummies were often perfectly collinear with the new entrant dummies, so we opted to discard the business type dummies.

[12] It would be possible to reap an efficiency gain by pooling these regressions and using a random effects Probit estimator. If this were done in the manner suggested by Chamberlain (1980), it would also be possible to allow for effects that are correlated with the explanatory variables. Our intuition is that these extensions will matter more for the coefficients of the Probit than for the Mill's ratio, so we have not pursued them.

fifth, sixth, and seventh difference equations.[13] As predicted by recent theories of industrial evolution, large firms are significantly less likely to exit in every year for almost all industries. Notice that from Stigler's (1958) perspective, the coefficient on our firm size proxy is itself an indicator of the importance of scale economies. In fact it is probably a *better* indicator than the one Stigler used because it describes the behavior of individual firms rather than that of the size distribution.[14] We will return to this point later. Finally, although the number of new entrants (reported as n80 and n81) is typically too small to permit accurate estimation of β_{80} or β_{81}, when estimates of these coefficients *are* significant they are always positive.

The size dependence of survival rates does not itself imply that production function estimates are biased. To address this question, we use the Probit results to construct Mills ratios for sample-selection corrections of the OLS estimates of the individual long-difference equations. Table 7 shows the increase in estimated RTS for each long difference equation when the sample-selection correction is employed: the corrections are almost uniformly quite small. Hence, unlike in Olley and Pakes (1990), our results based on extended samples do not appear to require further correction. This contrast with Olley and Pakes could be due to the fact that their estimator exploited both between and within variation, whereas our estimators remove time-invariant plant effects entirely.[15]

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[13] We focus on the long differences because, in addition to the reasons given in Section 4C below, they are most likely to be contaminated by selectivity bias. Note that whether a plant was a new entrant in 1979 cannot be discerned from our sample. Also, we are only able to analyse new entrants for the years 1980 and 1981 because plants did not report capital stocks after those years. (Stocks were imputed using investment data and 1980 or 1981 stocks for all plants in existance by 1981.) Finally, the sample sizes reported in Table 6 are different from those reported in earlier tables because the frequency of missing data for employment levels differs from that of variables used in previous regressions.

[14] Nonetheless, like cross-sectional RTS estimators, it is contaminated by Demsetz effects if inherently efficient plants last longer and grow bigger. This is presumably one reason our size coefficients are almost all negative.

[15] That is, the model used by Olley and Pakes will pick up selectivity effects if failing firms systematically differ from others in terms of efficiency levels, whereas our model is only sensitive to selectivity bias if failing plants exhibit lower or higher incremental output per unit

C. Robust Estimators

We now turn to estimates obtained with the GMM estimator. If the instruments at our disposal (machinery and equipment growth, real wage rates, and employment growth) are valid, these results are robust with respect to measurement error in the capital stock, heteroskedasticity, and selectivity bias.[16] For several reasons we hereafter limit the analysis to estimators that pool only the longer differences (i.e., fifth, sixth, and seventh differences).[17] First, gestation lags in capital stocks probably make the association between the true flow of capital services and measured changes in capital particularly weak over short periods. Second, by limiting the analysis to long differences we effectively leapfrog the severe recession that bottomed out during 1982 and 1983.[18] This is desirable because rapidly shrinking indutries are likely to have extreme excess capacity, and our instruments probably do not do an adequate job of recovering the true flow of capital services.

Overview of the Results

Findings for all 3-digit industries with sufficient data are reported in Table 8. Before discussing our findings regarding returns to scale, several observations are in order. First it appears that the GMM estimators do lessen measurement error bias. In particular, earlier

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incremental input.

[16] Finally, to reduce the problem of selectivity bias that was discussed in the previous seciton, we are working with the extended sample, *i.e.*, all plants for which data are available in the relevant years. Corresponding results for the balanced sample are contained in Table 8A in the Appendix.

[17] The sample sizes given in Table 8 are for the number of firms that appear in at least one of the equations involved in the GMM estimator. The samples differ somewhat from the samples employed for the simple difference estimators because the frequency of missing values for the instrumental variables differs from that of the variables being instrumented.

[18] Only two of the three fifth difference equations are used in the estimator that encompasses all long differnces. This is because the remaining fifth difference equation is redundant when both sixth difference equations are included.

discussion suggests that the elimination of measurement error should increase the coefficient on capital. This is precisely the pattern we find when comparing estimates based on seventh differences in Table 8 with the those in Table 4.[19] Theory also predicts that, on average, returns to scale estimates should rise when measurement error is eliminated. We find that this effect occurs in nine of the 16 industries when going from OLS to GMM estimates. The RTS pattern is not as strong as that for the capital coefficient because it is counterbalanced by decreases in the coefficient on labor (*cf.* equations 3.5 and 3.6).

However, one troubling feature of the GMM results remains. Recall that we attributed the systematic distinctions across simple difference estimators in Table 4 to biases induced by measurement error and/or selectivity problems. The GMM estimator applied to the extended sample (with and without Mills ratio corrections) is designed to eliminate these sources of bias, thus eliminating the systematic association between the sample period and estimated RTS. Nonetheless, RTS estimates based on seventh differences, on pooled sixth and seventh differences, and on pooled fifth, sixth and seventh differences vary considerably.[20] Similar results emerge when we apply the GMM estimator to those 4-digit industries for which we have adequate data (Table 9). This finding could mean that estimates exploiting fifth and sixth differences are relatively sensitive to biases deriving from gestation lags and lingering effects of the recession. Whatever the explanation, it appears that the instruments are not always effective in the fifth and sixth differences. So we focus on the seventh difference estimators in most of what follows, sacrificing degrees of freedom for apparent reductions in bias.

<u>Returns to Scale</u>

We have already seen that implausibly low returns to scale result from simple

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[19] This comparisons is made for seventh differences in order to hold the sample composition constant across estimators.

[20] Griliches and Marisse (1988) report a similar finding in their three-country study of manufacturing sector panel data.

"difference" and "within" estimators. Are the seventh difference GMM estimates similarly low? Table 10, which ranks industries by RTS, reveals that some clearly are. However, with but one exception, the industries with low RTS estimates are suspect because their average rate of value-added growth was less than negative 40 percent.[21] As already noted, rapidly shrinking industries are likely to have extreme excess capacity, and our instruments are unlikely to correct for the discrepancy between true and measured capital flows. Leaving these rapidly shrinking groups aside, the other RTS estimates are fairly evenly distributed over the plausible range of .8 to 1.2, and *none* is more than two standard deviations from constant RTS (refer back to Table 8). Also, unlike other estimators that are based on temporal variation in the data (*cf.* Tables 2 and 3), the relative elasticities of output with respect to labor and capital seem closer to those one might infer from factor shares under the assumption of competitive profit maximization.[22]

Table 9 reports estimates for more disaggregated (4-digit) industries. These provide additional details on the particular products generating increasing returns, and are less subject to the aggregation biases caused by heterogeneous products (via price deflators) and technologies (via variable coefficients). Results at the 4-digit level may also be useful in assessing the plausibility of changing composition of 3-digit industries as the explanation for within-industry heterogeneity over time, which was mentioned above. Here, note that structural metal products (*e.g.*, bridges, container tanks, metal door frames) are partly responsible for the high rank of metal products, and automobiles are partly responsible for increasing returns in transportation equipment. These findings square well with what is known about technology in these sectors. Notice also that some sectors that show decreasing returns at the 3-digit level show increasing returns in particular products. Notably, meatpacking,

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[21] The excepted industry (non-electric machinery, 382) has only 25 observations.

[22] Capital's share in value added for the manufacturing sector as a whole was in the neighborhood of .6 to .7 during the sample period.

scafood processing, and bakeries are sources of scale economies although the food industry (312) shows overall RTS slightly below unity. Also, while the textile industry shows decreasing returns overall, knitting shows scale economies. We caution however, that only two of the ten non-suspect industries show RTS significantly different from unity.

Our methodology is designed to reveal the plant-level scale effects that are realized as industries move through business cycles and regime changes. Hence, unlike engineering studies, the estimates do not capture sunk start-up costs, and they do not necessarily reflect the scale economies that might be reaped if existing plants were torn down and replaced with bigger ones. Nonetheless, it is interesting to ask whether there is some correspondence between the ranking of industries according to our estimates and rankings based on engineering studies of firms in industrialized countries. The latter tend to find that scale economies are most important in automobiles, certain metal products, iron/steel, electric machinery, and chemicals.[23] Referring again to Table 10, it is noteworthy that (after excluding suspect industries) transportation equipment, metal products, and electric machinery are ranked among the top five in our estimates as well.

Finally, we may test the plausibility of our GMM estimates by asking whether those industries where failure probabilities fall most rapidly with plant size are also the ones with the highest estimated RTS. To this end we look at the Spearman rank correlation coefficient between seventh difference (79-86) estimates of $-\beta_{TL}$ from Table 6 and seventh difference RTS estimates from Table 8 (or Table 10). Remarkably, this coefficient is .69 with a "t" ratio of 4.41 when all fifteen industries are used, and .80 with a "t" ratio of 5.37 when the four "suspect"

[23] This list is based on Pratten's (1990) survey, Berry's (1989) survey, and Scherer and Ross's (1990) summary of engineering studies. In summarizing their rankings, we have ignored industries that could not be analysed in our study for lack of data or because of rapid shrinkage.

industries are excluded.[24] It is tempting to conclude that our version of Stigler's survivor test has considerable empirical validity, and that it provides strong support for the GMM estimates. The alternative interpretation is that some feature of our research design has induced a spurious association between β_{TL} and RTS estimates. However, it is not obvious to us what this might be, as we have tested our results for selectivity bias and found them to be robust.

5. CONCLUSIONS

This study is the first we are aware of to provide systematic panel-based econometric estimates of the plant-level returns to scale in LDC manufacturing industries.[25] As such, we believe it sheds new light on several issues of interest to policy-makers. The first is whether increases in plant size cause efficiency improvements. If such causality is present over the production ranges in which plants operate, there are productivity gains associated with policies that promote "bigness" in manufacturing plants. On this issue, we find that although several 4-digit sectors show increasing returns, general expansion of the manufacturing sector cannot be expected to yield strong plant-level scale economies.[26] Specifically, if we take our "best" estimates at face value, they imply that the returns to scale in manufacturing are scattered across the range of .8 to 1.2 at the 3-digit level, and .7 to 1.6 at the 4-digit level. None of the 3-digit RTS estimates is significantly different from unity, and only two of the 4-digit

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[24] The correlations reported here are based on three-digit industries appearing both in Table 6 and Table 10. It was not possible to estimate our Probit model for transport equipment (384), given the small number of exiting plants. Nor did we estimate the production technology for miscellaneous manufacturing (390).

[25] There do exist cross-sectional analyses of returns to scale based on industrial census data. For the Chilean case, see Mellor (1975); Corbo and Mellor (1979); Tybout (forthcoming); and Tybout, de Melo and Corbo (forthcoming).

[26] It would be possible to ask whether the particular production shifts that accompanied Chile's opening to foreign competition resulted in better exploitation of scale economies. However, to do this properly, one needs to recognize the possibility of returns to scale in the production of intermediate inputs, for which input-analysis is necessary.

estimates are.

The second issue we address is whether efficiency causes plant growth, as Demsetz and others have argued. An affirmative answer means that positive correlations between size and profitability need not constitute a case for anti-trust activity. By comparing technology estimators that control for plant-specific efficiency effects with those that do not, we find evidence that Demsetz effects are indeed important. A corollary to this finding is that most returns to scale estimates based on cross-sectional data tend to overstate plant-level returns to scale as we have defined it here.

As a by-product, our analysis appears to have re-opened the possibility of using Stigler's survival test as a quick first pass on the importance of returns to scale. However, unlike earlier applications of this test based on the plant size distribution, our results suggest using the sensitivity of failure probabilities to plant size as an index of RTS.

The methodology developed herein appears to yield sensible results in many sectors, but some industries suffer from too few observations or too rapid shrinkage to permit reasonable inference. These problems could be reduced with more attention to the details of each sector and their implications for choice of instruments.

Appendix 1: The Generalized Method of Moments (GMM) Estimator

The GMM estimator can be used to pool information from the T-1 first-difference equations (based on periods 1 and 2, periods 2 and 3, etc.), the T-2 second-difference equations (based on periods 1 and 3, periods 2 and 4, etc.), the T-3 third-difference equations, and so on.[27] To see how this is done, imagine that we organize the data into blocks of n observations, one block corresponding to each of these $H = T(T-1)/2$ equations.[28] We may then define the explanatory variable matrix to be $\mathbb{X} = (\mathbb{X}_1', \mathbb{X}_2', .\ \mathbb{X}_h', ..\mathbb{X}_H')'_{(nH \times 2)}$, where if the h^{th} block corresponds to the j^{th} difference ending in period t, its representative row is (d^jL_{it}, d^jK_{it}). Similarly, output changes may organized into the vector $\mathbb{Y} = (\mathbb{Y}_1', \mathbb{Y}_2', .., \mathbb{Y}_h, ..., \mathbb{Y}_H')'$, with representative row for the h^{th} block (d^jY_{it}). Finally, by equation (3.4) the associated disturbance vector is $\mathbb{V} = (\mathbb{V}_1', ..\mathbb{V}_h', .., \mathbb{V}_H')'_{(nH \times 1)}$ where $\mathbb{V}_h = d^je_t - \beta d^jv_t$ has representative element $(d^je_{it} - \beta d^jv_{it})$.

Given the availability of appropriate instruments, the correlation between $\mathbb{X}_h$ and $\mathbb{V}_h$ induced by measurement error can be removed. Suppose $\mathbb{Z}_h$ is the $(n \times r_h)$ matrix of instrumental variables available for the h^{th} difference equation. ($\mathbb{Z}_h$ has representative row z_{ih} and each column of $\mathbb{Z}_h$ is orthogonal to $\mathbb{V}_h$.) Then defining $m = \Sigma r_h$ and $\mathbb{Z}_{(nH \times m)} = diag[\mathbb{Z}_h]$, $(h = 1, ..., H)$, the m orthogonality conditions $E\{\mathbb{Z}'\mathbb{V}\} = 0$ form the basis of the GMM estimator that efficiently exploits all the information in the data.

To construct the GMM estimator define $\mathbb{U}_{(m \times m)} = (1/n) \sum_{i=1}^{n} z_i'\hat{v}_i\hat{v}_i'z_i$ where $z_{i\ (H \times m)} = diag(z_{i1}, z_{i2}, ..z_{iH})$ and $\hat{v}_{i\ (H \times 1)}$ is a vector of residuals from the H equations for the i^{th} firm obtained with some consistent initial estimator (*e.g.*, two-stage least squares) or an iterative

[27] Although shorter difference estimators will be discarded in this paper's application, it is convenient to leave them in for the present exposition.

[28] For expositional purposes we ignore entry and exit and assume that n plants are observed in all periods. Arellano and Bond (1988) present the necessary modifications for the case of unbalanced panels. Also, for now we assume that enough equation-specific instruments are available to permit the use of all H equations.

procedure. Then the coefficient estimator is:

$$(\hat{\alpha}, \hat{\beta})' = [X'ZU^{-1}Z'X]^{-1}X'ZU^{-1}Z'Y,$$

and its covariance matrix is estimated by $n[X'ZU^{-1}Z'X]^{-1}$.

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Table 1

Total and Within Estimators of Cobb-Douglas Technology
(Balanced Sample)

$$Y = \alpha L + \beta K^{*} + \epsilon$$

Sector	Estimator	df	$\hat{\alpha}$ (t ratio)	$\hat{\beta}$ (t ratio)	$\hat{RTS}$ (std. error)	
	Total	5366	0.7937 (45.55)	0.3961 (37.05)	1.1898	0.0118
312	Within	4694	0.5303 (19.98)	0.1399 (5.22)	0.6702	0.0349
	Total	333	1.1164 (17.20)	0.2296 (5.69)	1.3460	0.0464
313	Within	290	0.6023 (5.68)	0.0564 (0.50)	0.6587	0.1512
	Total	1254	0.7178 (24.64)	0.2686 (12.37)	0.9864	0.0168
321	Within	1096	0.4384 (8.40)	0.2301 (4.49)	0.6685	0.0670
	Total	1054	0.9957 (32.28)	0.1254 (5.07)	1.1211	0.0233
322	Within	921	0.5942 (10.30)	0.1502 (2.76)	0.7444	0.0712
	Total	454	0.9615 (18.19)	0.1770 (5.21)	1.1385	0.0291
324	Within	396	0.6019 (6.65)	O.2274 (2.67)	0.8293	0.1091
	Total	846	0.7865 (16.65)	0.2731 (8.33)	1.0626	0.0325
331	Within	739	0.5330 (6.18)	0.2700 (2.64)	0.8030	0.1205
	Total	310	0.8746 (12.01)	0.3956 (8.49)	1.2702	0.0448
332	Within	260	0.6002 (4.79)	0.1873 (1.81)	0.7875	0.1512
	Total	619	0.6874 (20.03)	0.3331 (12.86)	1.0205	0.0206
342	Within	540	0.3640 (6.40)	0.2202 (3.00)	0.5842	0.0807
	Total	590	0.7043 (15.51)	0.4007 (12.28)	1.1049	0.0285
352	Within	515	0.2521 (4.62)	0.1674 (2.71)	0.4195	0.0750
	Total	230	0.7097 (12.09)	0.3699 (7.62)	1.0796	0.0388
355	Within	200	0.3076 (3.30)	0.3413 (3.04)	0.6489	0.1286
	Total	366	0.6069 (9.42)	0.2651 (6.15)	0.8720	0.0443
356	Within	319	0.3351 (2.53)	0.5210 (4.59)	0.8651	0.1459
	Total	302	0.7549 (11.95)	0.3129 (9.68)	1.0678	0.0394
369	Within	263	0.5090 (5.46)	0.1111 (1.42)	0.6201	0.1139
	Total	1110	0.9404 (29.31)	0.2272 (10.27)	1.1676	0.0219
381	Within	970	0.4515 (7.99)	0.1472 (3.00)	0.5987	0.0693

Table 1 (continued)

Sector	Estimator	df	$\hat{\alpha}$ (t ratio)	$\hat{\beta}$ (t ratio)	$\hat{RTS}$ (std. error)	
	Total	261	0.7406 (12.60)	0.3437 (8.65)	1.0843	0.0454
382	Within	327	0.2831 (3.42)	0.0709 (0.85)	0.3540	0.1011
	Total	182	0.6470 (8.76)	0.4011 (7.17)	1.0481	0.0427
383	Within	158	0.4778 (4.49)	0.2746 (1.86)	0.7524	0.1719
	Total	230	0.8780 (10.66)	0.2341 (3.90)	1.1121	0.0462
384	Within	200	0.6989 (5.52)	0.2916 (2.05)	0.9905	0.1571

Table 2: Evidence on Serial Correlation in Measurement Error (Balanced Sample)

Part A: Autocorrelation Coefficients for Observed Capital Stocks

Industry

Autocorrelation Coefficient	312	313	321	322	324	331	332	342
ρ_1	0.9881	0.9873	0.9883	0.9729	0.9906	0.9875	0.9872	0.9910
ρ_2	0.9772	0.9714	0.9762	0.9511	0.9791	0.9758	0.9709	0.9821
ρ_3	0.9642	0.9608	0.9662	0.9284	0.9670	0.9628	0.9572	0.9730
ρ_4	0.9482	0.9535	0.9560	0.9098	0.9569	0.9488	0.9443	0.9628
ρ_5	0.9345	0.9382	0.9427	0.8799	0.9441	0.9368	0.9231	0.9538
ρ_6	0.9175	0.9149	0.9212	0.8474	0.9281	0.9216	0.8988	0.9450
ρ_7	0.8901	0.8848	0.9024	0.7981	0.9116	0.8930	0.8398	0.9257
	352	355	356	369	381	382	383	384
ρ_1	0.9847	0.9816	0.9801	0.9897	0.9800	0.9759	0.9938	0.9893
ρ_2	0.9671	0.9647	0.9610	0.9804	0.9610	0.9565	0.9836	0.9758
ρ_3	0.9462	0.9461	0.9453	0.9746	0.9447	0.9258	0.9705	0.9607
ρ_4	0.9283	0.9329	0.9251	0.9686	0.9257	0.8883	0.9567	0.9470
ρ_5	0.9059	0.9145	0.8928	0.9623	0.9007	0.8649	0.9397	0.9260
ρ_6	0.8826	0.8817	0.8561	0.9512	0.8737	0.8420	0.9272	0.9051
ρ_7	0.8538	0.8384	0.7995	0.9393	0.8226	0.7986	0.9129	0.8762

Part B: Consistent Point Estimates of γ Coefficient from Auxiliary Regression

Industry

Estimator	312	313	321	322	324	331	332	342
Within	0.1447	0.0413	0.1647	0.2046	0.2417	0.1616	0.1653	0.1956
1^{st} Difference	0.0364	-.0112	0.0291	0.0451	0.0099	0.0589	0.0033	0.0934
2^{nd} Difference	0.0880	0.0069	0.1091	0.1406	0.1080	0.1353	0.0796	0.1559
3^{rd} Difference	0.1246	0.0572	0.1529	0.2071	0.2142	0.1973	0.0982	0.1864
4^{th} Difference	0.1766	0.0655	0.1868	0.2220	0.3132	0.2173	0.2080	0.2293
5^{th} Difference	0.2023	0.0343	0.2234	0.2885	0.3387	0.2031	0.2537	0.2883
6^{th} Difference	0.2394	0.0303	0.2719	0.3127	0.3933	0.1536	0.3502	0.2627
7^{th} Difference	0.2471	0.1401	0.3530	0.3248	0.3576	0.1147	0.4755	0.2733
	352	355	356	369	381	382	383	384
Within	0.1543	0.1876	0.3562	0.1472	0.1646	0.2581	0.0828	0.2863
1^{st} Difference	0.0448	0.0537	0.1124	0.1251	0.0211	0.0361	0.0168	0.0945
2^{nd} Difference	0.1005	0.0641	0.2067	0.1321	0.1193	0.1626	0.0455	0.1250
3^{rd} Difference	0.1464	0.1421	0.1766	-.0140	0.1297	0.2106	0.0731	0.2392
4^{th} Difference	0.1635	0.1844	0.3512	0.1454	0.1594	0.2900	0.1197	0.2710
5^{th} Difference	0.1887	0.2718	0.5229	0.1756	0.2344	0.3762	0.1593	0.3820
6^{th} Difference	0.2310	0.3382	0.7275	0.2913	0.2913	0.6077	0.1171	0.4471
7^{th} Difference	0.3341	0.4921	1.1180	0.2990	0.4405	0.8402	0.0732	0.4764

Table 2 (continued)

Part C: Variances of Differenced Log-Capital: var(d^jK)

Industry

j	312	313	321	322	324	331	332	342
1	0.0635	0.0780	0.0679	0.0855	0.0586	0.0529	0.0530	0.0551
2	0.1203	0.1800	0.1346	0.1532	0.1298	0.1007	0.1179	0.1063
3	0.1895	0.2566	0.1909	0.2269	0.2010	0.1523	0.1836	0.1621
4	0.2735	0.3109	0.2504	0.2941	0.2545	0.2103	0.2392	0.2252
5	0.3492	0.4187	0.3304	0.3991	0.3306	0.2664	0.3389	0.2753
6	0.4433	0.6156	0.4564	0.5217	0.4175	0.3454	0.4684	0.3394
7	0.6010	0.9618	0.5612	0.7145	0.5405	0.4918	0.7487	0.4720

j	352	355	356	369	381	382	383	384
1	0.0615	0.0612	0.0797	0.1478	0.0967	0.1016	0.0288	0.0627
2	0.1319	0.1181	0.1510	0.2727	0.1858	0.1817	0.0762	0.1394
3	0.2171	0.1767	0.2249	0.3582	0.2634	0.3131	0.1369	0.2188
4	0.2907	0.2174	0.2969	0.4424	0.3559	0.4724	0.2014	0.2962
5	0.3814	0.2650	0.4017	0.5546	0.4767	0.5738	0.2830	0.3852
6	0.4768	0.3687	0.4851	0.7492	0.6062	0.6502	0.3505	0.4699
7	0.6015	0.5009	0.5383	1.0946	0.8234	0.9517	0.4459	0.5900

Table 3: Simple Difference Estimators by 3-Digit Industry (Balanced Sample)

Industry 312

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	4695	0.4408	(14.92)	0.0999	(2.36)	0.5407
2nd Difference	4024	0.4766	(15.31)	0.1633	(4.31)	0.6399
3rd Difference	3353	0.5524	(17.31)	0.1616	(4.71)	0.7140
4th Difference	2684	0.5817	(17.04)	0.1518	(4.67)	0.7335
5th Difference	2011	0.5443	(14.40)	0.1226	(3.65)	0.6669
6th Difference	1340	0.5957	(13.73)	0.1067	(2.94)	0.7024
7th Difference	671	0.5593	(10.00)	0.1335	(3.03)	0.6928

Industry 313

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	291	0.3535	(2.57)	0.0497	(0.25)	0.4032
2nd Difference	249	0.3257	(2.57)	0.0416	(0.30)	0.3673
3rd Difference	207	0.5013	(3.79)	0.1701	(1.30)	0.6714
4th Difference	165	0.6577	(4.81)	0.0794	(0.57)	0.7371
5th Difference	123	0.5815	(4.15)	0.0425	(0.28)	0.6240
6th Difference	81	0.8039	(5.21)	-0.1924	(-1.25)	0.6115
7th Difference	39	1.2323	(6.85)	0.1491	(0.90)	1.3814

Industry 321

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	1097	0.2980	(5.08)	0.1405	(1.78)	0.4385
2nd Difference	940	0.3673	(6.11)	0.1754	(2.55)	0.5427
3rd Difference	783	0.4489	(7.53)	0.1984	(3.14)	0.6473
4th Difference	626	0.4827	(7.30)	0.2465	(3.78)	0.7292
5th Difference	469	0.5237	(6.73)	0.2634	(4.01)	0.7871
6th Difference	312	0.5315	(6.25)	0.2637	(4.00)	0.7952
7th Difference	155	0.5161	(4.16)	0.2292	(2.48)	0.7453

Industry 322

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	922	0.2631	(3.94)	0.1242	(1.53)	0.3873
2nd Difference	790	0.4647	(6.88)	0.0785	(1.06)	0.5432
3rd Difference	658	0.5291	(7.57)	0.1220	(1.73)	0.6511
4th Difference	526	0.7155	(10.00)	0.1087	(1.60)	0.8242
5th Difference	394	0.7832	(9.85)	0.1894	(2.88)	0.9726
6th Difference	262	0.7453	(8.02)	0.1735	(2.40)	0.9188
7th Difference	130	0.9427	(8.48)	0.1348	(1.66)	1.0775

Industry 324

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	397	0.2102	(2.05)	0.1728	(1.30)	0.3830
2nd Difference	340	0.4673	(4.35)	0.1832	(1.62)	0.6505
3rd Difference	283	0.6884	(5.97)	0.1879	(1.80)	0.8763
4th Difference	226	0.6741	(5.69)	0.2066	(1.98)	0.8807
5th Difference	169	0.5988	(4.83)	0.3088	(2.85)	0.9076
6th Difference	112	0.7691	(5.55)	0.1738	(1.45)	0.9429
7th Difference	55	0.9458	(5.10)	0.1638	(1.01)	1.1096

Industry 331

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	740	0.4531	(4.46)	0.2076	(1.29)	0.6607
2nd Difference	634	0.5228	(5.17)	0.2121	(1.52)	0.7349
3rd Difference	528	0.5584	(5.15)	0.2843	(2.12)	0.8427
4th Difference	422	0.6165	(5.72)	0.2730	(2.21)	0.8895
5th Difference	316	0.5586	(4.88)	0.3359	(2.69)	0.8945
6th Difference	210	0.5191	(3.89)	0.2974	(2.11)	0.8165
7th Difference	104	0.3634	(1.88)	0.1734	(1.05)	0.5368

Industry 332

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	271	0.3977	(2.69)	0.2333	(1.26)	0.6310
2nd Difference	232	0.4574	(3.62)	0.1606	(1.25)	0.6180
3rd Difference	193	0.6931	(4.93)	0.1026	(0.76)	0.7957
4th Difference	154	0.6071	(3.79)	0.2201	(1.66)	0.8272
5th Difference	115	0.8513	(4.90)	0.1569	(1.29)	1.0082
6th Difference	76	0.6666	(2.85)	0.2257	(1.58)	0.8923
7th Difference	37	0.5607	(2.04)	0.1953	(1.30)	0.7560

Industry 342

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	540	0.2646	(4.20)	0.1217	(1.01)	0.3863
2nd Difference	462	0.3916	(6.55)	0.1704	(1.77)	0.5620
3rd Difference	384	0.4218	(6.21)	0.2227	(2.52)	0.6445
4th Difference	307	0.2459	(3.17)	0.3013	(3.45)	0.5472
5th Difference	230	0.3393	(3.80)	0.2342	(2.47)	0.5735
6th Difference	152	0.4183	(4.11)	0.2023	(1.81)	0.6206
7th Difference	74	0.5883	(6.02)	0.1845	(1.74)	0.7728

Industry 352

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	516	0.0419	(0.72)	0.0327	(0.33)	0.0746
2nd Difference	442	0.1780	(2.94)	0.1423	(1.77)	0.3203
3rd Difference	368	0.2290	(3.57)	0.1455	(1.99)	0.3745
4th Difference	294	0.3488	(4.93)	0.1391	(1.83)	0.4879
5th Difference	220	0.3899	(4.75)	0.1579	(1.89)	0.5478
6th Difference	146	0.3781	(4.04)	0.2475	(2.71)	0.6256
7th Difference	72	0.2053	(1.51)	0.2014	(1.65)	0.4067

Industry 355

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	201	0.3183	(3.09)	0.2079	(1.37)	0.5262
2nd Difference	172	0.1649	(1.52)	0.2276	(1.56)	0.3925
3rd Difference	143	0.2307	(2.14)	0.2611	(1.89)	0.4918
4th Difference	114	0.2521	(2.18)	0.3319	(2.31)	0.5840
5th Difference	85	0.3953	(2.96)	0.4466	(2.83)	0.8419
6th Difference	56	0.4748	(2.84)	0.3898	(2.34)	0.8646
7th Difference	27	0.5682	(2.30)	0.3408	(1.62)	0.9090

Industry 356

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	320	0.1934	(1.37)	0.0809	(0.48)	0.2743
2nd Difference	274	0.3026	(2.15)	0.2533	(1.72)	0.5559
3rd Difference	228	0.3489	(2.32)	0.5225	(3.62)	0.8714
4th Difference	182	0.3984	(2.44)	0.6528	(4.52)	1.0512
5th Difference	136	0.3897	(1.98)	0.6586	(4.55)	1.0483
6th Difference	90	0.1938	(0.72)	0.6778	(3.78)	0.8716
7th Difference	44	0.6187	(1.49)	0.3840	(1.64)	1.0027

Industry 369

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	264	0.3069	(3.03)	0.1200	(1.18)	0.4269
2nd Difference	226	0.4049	(4.07)	0.1895	(2.15)	0.5944
3rd Difference	188	0.4858	(4.41)	0.1133	(1.22)	0.5991
4th Difference	150	0.5530	(4.35)	0.2211	(2.10)	0.7741
5th Difference	112	0.5946	(4.09)	0.0380	(0.32)	0.6326
6th Difference	74	0.6108	(3.71)	-0.0387	(-0.31)	0.5721
7th Difference	36	0.7923	(4.13)	0.0753	(0.54)	0.8676

Industry 381

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	971	0.2912	(4.34)	0.1637	(2.07)	0.4549
2nd Difference	832	0.3283	(5.39)	-0.0100	(-0.16)	0.3183
3rd Difference	693	0.4805	(7.25)	0.0197	(0.31)	0.5002
4th Difference	554	0.4527	(6.42)	0.1564	(2.55)	0.6091
5th Difference	415	0.5315	(6.40)	0.1863	(3.04)	0.7178
6th Difference	276	0.6700	(7.12)	0.2330	(3.64)	0.9030
7th Difference	137	0.6271	(5.14)	0.2497	(3.11)	0.8768

Industry 382

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1th Difference	228	0.0798	(1.03)	0.0587	(0.47)	0.1385
2th Difference	195	0.2926	(3.21)	0.0647	(0.54)	0.3573
3th Difference	162	0.2973	(3.28)	-0.0032	(-0.03)	0.2941
4th Difference	129	0.3667	(3.20)	0.0532	(0.50)	0.4199
5th Difference	96	0.3612	(2.79)	0.0527	(0.48)	0.4139
6th Difference	63	0.4457	(2.67)	0.0630	(0.53)	0.5087
7th Difference	30	0.2964	(1.13)	0.1181	(0.78)	0.4145

Industry 383

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	159	0.2143	(1.68)	0.0020	(0.01)	0.1040
2nd Difference	136	0.4268	(3.49)	0.2990	(1.33)	0.7258
3rd Difference	113	0.7028	(5.33)	0.1644	(0.83)	0.8672
4th Difference	90	0.4923	(3.42)	0.3927	(2.15)	0.8850
5th Difference	67	0.4172	(2.90)	0.2639	(1.73)	0.6811
6th Difference	44	0.5934	(4.71)	0.2782	(1.92)	0.8716
7th Difference	21	0.4164	(2.09)	0.2986	(1.29)	0.7150

Industry 384

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	201	0.6261	(4.00)	0.0924	(0.39)	0.7185
2nd Difference	172	0.7574	(5.24)	0.0288	(0.16)	0.7862
3rd Difference	143	0.6610	(4.08)	0.2394	(1.31)	0.9004
4th Difference	114	0.6664	(4.03)	0.3637	(2.02)	1.0301
5th Difference	85	0.7266	(4.43)	0.4299	(2.51)	1.1565
6th Difference	56	0.4165	(1.96)	0.4626	(2.12)	0.8791
7th Difference	27	1.1125	(4.63)	0.1511	(0.60)	1.2636

Table 4: Simple Difference Estimators by 3-Digit Industry (Extended Sample)

Industry 312

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	6757	0.4551	(18.73)	0.1258	(3.16)	0.5809
2nd Difference	5547	0.5088	(19.49)	0.1742	(4.92)	0.6830
3rd Difference	4403	0.5627	(20.21)	0.1620	(4.94)	0.7247
4th Difference	3351	0.5817	(19.47)	0.1469	(4.75)	0.7286
5th Difference	2378	0.5735	(16.87)	0.1181	(3.68)	0.6916
6th Difference	1487	0.6054	(14.94)	0.1115	(3.19)	0.7169
7th Difference	669	0.5593	(10.00)	0.1335	(3.03)	0.6928

Industry 313

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	488	0.2347	(2.63)	0.1258	(0.70)	0.3605
2nd Difference	391	0.3098	(3.04)	0.0501	(0.35)	0.3599
3rd Difference	302	0.4000	(3.70)	0.1764	(1.32)	0.5764
4th Difference	222	0.5543	(4.61)	0.0349	(0.24)	0.5892
5th Difference	154	0.4483	(3.29)	0.0385	(0.24)	0.4868
6th Difference	94	0.7867	(5.20)	-0.2027	(-1.27)	0.5840
7th Difference	39	1.2323	(6.85)	0.1491	(0.90)	1.3814

Industry 321

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	1926	0.3272	(6.18)	0.0599	(0.81)	0.3871
2nd Difference	1529	0.4192	(7.45)	0.1251	(1.88)	0.5443
3rd Difference	1173	0.5439	(9.15)	0.1181	(1.83)	0.6620
4th Difference	864	0.6158	(9.89)	0.1987	(3.17)	0.8145
5th Difference	599	0.6054	(8.53)	0.2092	(3.42)	0.8146
6th Difference	362	0.5521	(6.70)	0.2541	(3.93)	0.8061
7th Difference	155	0.5161	(4.16)	0.2292	(2.48)	0.7453

Industry 322

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	1670	0.4330	(8.06)	0.1096	(1.43)	0.5426
2nd Difference	1306	0.4887	(8.75)	0.0782	(1.14)	0.5669
3rd Difference	985	0.5140	(8.86)	0.0650	(1.00)	0.5790
4th Difference	717	0.6472	(10.23)	0.0803	(1.23)	0.7275
5th Difference	498	0.7486	(10.48)	0.1744	(2.73)	0.9230
6th Difference	301	0.7198	(8.17)	0.1655	(2.35)	0.8853
7th Difference	130	0.9427	(8.48)	0.1348	(1.66)	1.0775

Industry 324

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	680	0.4809	(5.08)	0.1785	(1.39)	0.6594
2nd Difference	547	0.7030	(7.26)	0.2644	(2.38)	0.9674
3rd Difference	427	0.8056	(7.39)	0.2719	(2.52)	1.0775
4th Difference	316	0.7285	(7.40)	0.2783	(3.22)	1.0068
5th Difference	218	0.6207	(5.58)	0.3208	(3.40)	0.9415
6th Difference	131	0.7287	(5.88)	0.1822	(1.71)	0.9109
7th Difference	55	0.9458	(5.10)	0.1638	(1.01)	1.1096

Industry 331

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	1600	0.5723	(9.37)	0.2620	(3.00)	0.8343
2nd Difference	1250	0.5563	(8.50)	0.2136	(2.62)	0.7699
3rd Difference	936	0.5960	(8.16)	0.1833	(2.18)	0.7793
4th Difference	666	0.6840	(7.86)	0.0810	(0.90)	0.7650
5th Difference	445	0.6909	(7.45)	0.1329	(1.44)	0.8238
6th Difference	261	0.4876	(4.07)	0.0823	(0.66)	0.5699
7th Difference	104	0.3634	(1.88)	0.1734	(1.05)	0.5368

Industry 332

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	676	0.3289	(2.71)	0.3962	(2.47)	0.7251
2nd Difference	505	0.5311	(4.27)	0.3050	(2.23)	0.8361
3rd Difference	359	0.6400	(5.03)	0.1594	(1.24)	0.7994
4th Difference	248	0.7135	(4.92)	0.1638	(1.24)	0.8773
5th Difference	163	0.8442	(6.44)	0.0726	(0.68)	0.9168
6th Difference	96	0.7213	(4.14)	0.1434	(1.19)	0.8647
7th Difference	37	0.5607	(2.04)	0.1953	(1.30)	0.7560

Industry 342

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	971	0.2182	(4.32)	0.2714	(2.84)	0.4896
2nd Difference	780	0.2724	(5.75)	0.3127	(4.03)	0.5851
3rd Difference	603	0.3356	(6.20)	0.2876	(3.84)	0.6232
4th Difference	442	0.2350	(3.79)	0.3735	(4.92)	0.6085
5th Difference	304	0.3152	(3.99)	0.2990	(3.41)	0.6142
6th Difference	181	0.3812	(4.17)	0.2269	(2.23)	0.6081
7th Difference	74	0.5883	(6.02)	0.1845	(1.74)	0.7728

Industry 352

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	814	0.0680	(1.14)	0.0530	(0.67)	0.1210
2nd Difference	664	0.2134	(3.46)	0.1093	(1.69)	0.3227
3rd Difference	522	0.3046	(4.82)	0.1440	(2.26)	0.4486
4th Difference	395	0.3909	(5.94)	0.1307	(1.87)	0.5216
5th Difference	277	0.3821	(4.97)	0.0881	(1.12)	0.4702
6th Difference	169	0.3488	(3.92)	0.2257	(2.55)	0.5745
7th Difference	72	0.2053	(1.51)	0.2014	(1.65)	0.4067

Industry 355

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	294	0.3821	(4.64)	0.1012	(0.83)	0.4833
2nd Difference	234	0.3177	(3.35)	0.0925	(0.88)	0.4102
3rd Difference	182	0.3047	(3.22)	0.1876	(1.80)	0.4923
4th Difference	139	0.2594	(2.39)	0.3206	(2.95)	0.5800
5th Difference	98	0.3317	(2.78)	0.4684	(4.16)	0.8001
6th Difference	61	0.4873	(3.09)	0.4412	(3.31)	0.9285
7th Difference	27	0.5682	(2.30)	0.3408	(1.62)	0.9090

Industry 356

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	663	0.3153	(3.11)	0.3146	(2.40)	0.6299
2nd Difference	513	0.4090	(4.01)	0.3101	(2.61)	0.7191
3rd Difference	385	0.3809	(3.35)	0.5583	(4.44)	0.9392
4th Difference	276	0.3221	(2.45)	0.6070	(4.67)	0.9291
5th Difference	187	0.3167	(1.93)	0.6629	(5.06)	0.9796
6th Difference	106	0.2192	(0.85)	0.5884	(3.39)	0.8076
7th Difference	44	0.6187	(1.49)	0.3840	(1.64)	1.0027

Industry 369

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	$\hat{RTS}$
1st Difference	478	0.5685	(6.12)	-0.0235	(-0.27)	0.5450
2nd Difference	380	0.5750	(6.40)	0.0917	(1.16)	0.6667
3rd Difference	289	0.4351	(4.29)	-0.0089	(-0.11)	0.4262
4th Difference	207	0.6258	(5.32)	0.1540	(1.70)	0.7798
5th Difference	142	0.6210	(4.45)	-0.0352	(-0.33)	0.5860
6th Difference	85	0.6689	(3.82)	-0.0253	(-0.19)	0.6436
7th Difference	36	0.7923	(4.13)	0.0753	(0.54)	0.8676

Industry 381

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	R$\hat{T}$S
1st Difference	1859	0.3117	(6.39)	0.1236	(1.97)	0.4353
2nd Difference	1439	0.3916	(8.32)	0.0199	(0.35)	0.4115
3rd Difference	1085	0.5515	(10.36)	0.0467	(0.80)	0.5982
4th Difference	787	0.5346	(9.45)	0.1556	(2.83)	0.6902
5th Difference	544	0.6295	(9.18)	0.1379	(2.45)	0.7674
6th Difference	329	0.6723	(8.24)	0.2349	(3.86)	0.9072
7th Difference	137	0.6271	(5.14)	0.2497	(3.11)	0.8768

Industry 382

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	R$\hat{T}$S
1th Difference	548	0.3271	(5.03)	0.0057	(0.06)	0.3328
2th Difference	422	0.4791	(6.78)	0.0313	(0.34)	0.5104
3th Difference	314	0.4664	(6.26)	0.0625	(0.72)	0.5289
4th Difference	220	0.4577	(5.13)	0.0953	(1.05)	0.5530
5th Difference	143	0.3951	(3.44)	0.1038	(1.04)	0.4989
6th Difference	81	0.5219	(2.96)	0.0472	(0.37)	0.5691
7th Difference	30	0.2964	(1.13)	0.1181	(0.78)	0.4145

Industry 383

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	R$\hat{T}$S
1st Difference	279	0.4143	(2.95)	0.2298	(0.66)	0.6441
2nd Difference	218	0.6562	(4.65)	0.6693	(2.47)	1.3255
3rd Difference	165	0.6601	(5.22)	0.1382	(0.71)	0.7983
4th Difference	121	0.5468	(3.97)	0.2197	(1.19)	0.7665
5th Difference	85	0.3692	(2.96)	0.2533	(1.81)	0.6225
6th Difference	50	0.5980	(5.00)	0.2679	(1.93)	0.8659
7th Difference	21	0.4164	(2.09)	0.2986	(1.29)	0.7150

Industry 384

Estimator	df	$\hat{\alpha}$	(t ratio)	$\hat{\beta}$	(t ratio)	R$\hat{T}$S
1s^{t} Difference	475	0.5487	(5.61)	0.0155	(0.14)	0.5642
2nd Difference	360	0.6917	(6.79)	0.0780	(0.76)	0.7697
3rd Difference	258	0.6483	(5.51)	0.2732	(2.20)	0.9215
4th Difference	178	0.7180	(4.92)	0.2898	(2.34)	1.0078
5th Difference	118	0.5986	(3.94)	0.4882	(3.39)	1.0868
6th Difference	67	0.3961	(2.37)	0.4548	(2.88)	0.8509
7th Difference	27	1.1125	(4.63)	0.1511	(0.60)	1.2636

Table 5: Proportional Increase in Estimated RTS, Extended versus Balanced Sample

j	Industry 312	313	321	322	324	331	332	342
1	0.0743	-0.1059	-0.1172	0.4010	0.7217	0.2628	0.1491	0.2674
2	0.0674	-0.0201	0.0029	0.0436	0.4870	0.0476	0.3529	0.0411
3	0.0150	-0.1415	0.0227	0.1107	0.2290	-0.0752	0.0047	-0.0330
4	-0.0067	-0.2007	0.1170	-0.1173	0.1432	-0.1400	0.0606	0.1120
5	0.0370	-0.2199	0.0349	-0.0510	0.0374	-0.0790	-0.0907	0.0710
6	0.0206	-0.0450	0.0137	-0.0365	-0.0339	-0.3020	-0.0309	-0.0201
7	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

j	352	355	356	369	381	382	383	384
1	0.6220	-0.0815	1.2964	0.2766	-0.0451	1.4029	5.1933	-0.2148
2	0.0075	0.0360	0.2936	0.1216	0.2928	0.4285	0.8263	-0.0210
3	0.1979	0.0009	0.0778	-0.2886	0.1959	0.7984	-0.0795	0.0234
4	0.0691	0.0068	-0.1162	0.0074	0.1331	0.3170	-0.1339	-0.0216
5	-0.1417	0.0496	-0.0655	-0.0737	0.0691	0.2054	-0.0860	-0.0603
6	-0.0817	0.0739	-0.0734	0.1250	0.0047	0.1187	-0.0065	-0.0321
7	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Table 6: Probit Models of Plant Exit (Extended Sample)

Industry 312

diff.	n	nx	n80	n81	β_{80}	(t ratio)	β_{81}	(t ratio)	β_{TL}	(t ratio)
84-79	1607	461							-0.33	(-8.21)
85-80	1490	410	73		0.43	(2.82)			-0.31	(-7.08)
86-81	1417	394	58	80	0.39	(2.24)	0.05	(0.35)	-0.32	(-7.24)
85-79	1607	509							-0.33	(-8.32)
86-80	1490	503	73		0.47	(3.08)			-0.28	(-6.80)
86-79	1607	602							-0.29	(-7.87)

Industry 313

diff.	n	nx	n80	n81	β_{80}	(t ratio)	β_{81}	(t ratio)	β_{TL}	(t ratio)
84-79	210	91							-0.49	(-5.19)
85-80	185	73	12		0.51	(1.32)			-0.27	(-2.84)
86-81	158	59	6	9	0.98	(2.54)	...		-0.36	(-3.26)
85-79	210	101							-0.48	(-5.15)
86-80	185	88	12		1.24	(2.37)			-0.33	(-3.35)
86-79	210	113							-0.50	(-5.29)

Industry 321

diff.	n	nx	n80	n81	β_{80}	(t ratio)	β_{81}	(t ratio)	β_{TL}	(t ratio)
84-79	502	240							-0.15	(-2.92)
85-80	441	187	16		-0.08	(-0.25)			-0.27	(-4.49)
86-81	403	153	14	23	0.20	(0.59)	0.19	(0.70)	-0.29	(-4.49)
85-79	502	248							-0.19	(-3.61)
86-80	441	204	16		-0.02	(-0.08)			-0.28	(-4.73)
86-79	502	265							-0.21	(-4.04)

Industry 322

diff.	n	nx	n80	n81	β_{80}	(t ratio)	β_{81}	(t ratio)	β_{TL}	(t ratio)
84-79	442	232							-0.26	(-3.93)
85-80	396	199	12		0.63	(1.57)			-0.21	(-3.07)
86-81	344	157	7	10	0.17	(0.35)	-0.18	(-0.44)	-0.30	(-3.82)
85-79	442	249							-0.26	(-3.98)
86-80	396	210	12		0.55	(1.39)			-0.22	(-3.16)
86-79	442	261							-0.25	(-3.77)

Industry 324

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	184	83							-0.31	(-3.00)
85-80	154	65	6		0.07	(0.13)			-0.26	(-2.28)
86-81	136	52	5	4	0.45	(0.77)	0.13	(0.20)	-0.30	(-2.43)
85-79	184	94							-0.33	(-3.26)
86-80	154	71	6		0.38	(0.71)			-0.32	(-2.75)
86-79	184	98							-0.38	(-3.65)

Industry 331

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	522	286							-0.21	(-3.92)
85-80	447	216	29		-0.04	(-0.18)			-0.22	(-3.35)
86-81	406	189	22	33	0.08	(0.29)	0.14	(0.59)	-0.37	(-5.23)
85-79	522	293							-0.22	(-4.00)
86-80	447	243	29		0.17	(0.70)			-0.25	(-3.75)
86-79	522	319							-0.21	(-3.74)

Industry 332

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	210	125							-0.38	(-3.17)
85-80	191	114	9		-0.22	(-0.52)			-0.53	(-3.94)
86-81	169	100	9	5	-0.17	(-0.40)	0.13	(0.24)	-0.72	(-4.85)
85-79	210	133							-0.39	(-3.27)
86-80	191	122	9		-0.37	(-0.86)			-0.22	(-4.46)
86-79	210	143							-0.47	(-3.86)

Industry 342

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	242	104							-0.26	(-2.92)
85-80	226	89	8		0.32	(0.67)			-0.39	(-3.87)
86-81	205	81	8	3	0.28	(0.56)	0.21	(0.28)	-0.58	(-4.84)
85-79	242	110							-0.29	(-3.31)
86-80	226	99	8		0.18	(0.38)			-0.40	(-4.09)
86-79	242	118							-0.31	(-3.51)

Industry 352

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	171	47							-0.31	(-3.10)
85-80	165	39	5		0.18	(0.30)			-0.36	(-3.18)
86-81	157	39	5	3	0.17	(0.28)	0.71	(0.92)	-0.33	(-2.95)
85-79	171	50							-0.26	(-2.76)
86-80	165	44	5		0.02	(0.03)			-0.43	(-3.85)
86-79	171	56							-0.32	(-3.40)

Industry 355

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	63	18							0.10	(0.74)
85-80	67	22	3		0.92	(1.19)			-0.002	(-0.01)
86-81	58	18	3	0	6.30	(0.01)	...		0.13	(0.81)
85-79	63	21							0.03	(0.20)
86-80	67	27	3		...				0.09	(0.61)
86-79	63	24							0.19	(1.34)

Industry 356

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	170	63							-0.33	(-2.94)
85-80	162	55	8		0.58	(1.22)			-0.43	(-3.10)
86-81	149	44	6	3	0.44	(0.82)	0.67	(0.85)	-0.37	(-2.90)
85-79	170	66							-0.30	(-2.72)
86-80	162	58	8		0.50	(1.07)			-0.48	(-3.41)
86-79	170	70							-0.32	(-2.89)

Industry 369

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	135	53							-0.41	(-3.22)
85-80	138	50	11		0.13	(0.31)			-0.54	(-4.05)
86-81	127	43	10	3	0.36	(0.83)	-0.35	(-0.45)	-0.43	(-3.12)
85-79	135	53							-0.36	(-2.92)
86-80	138	55	11		0.07	(0.16)			-0.45	(-3.67)
86-79	135	58							-0.29	(-2.49)

Industry 381

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	458	224							-0.46	(-6.88)
85-80	445	204	16		0.19	(0.57)			-0.53	(-7.25)
86-81	411	183	13	21	0.30	(0.83)	0.44	(1.45)	-0.45	(-6.06)
85-79	458	233							-0.47	(-7.11)
86-80	445	220	16		0.28	(0.83)			-0.44	(-6.41)
86-79	458	248							-0.40	(-6.23)

Industry 382

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	169	85							-0.32	(-3.14)
85-80	140	53	5		0.44	(0.74)			-0.30	(-2.60)
86-81	144	66	3	6	0.56	(0.73)	0.56	(1.05)	-0.02	(-0.26)
85-79	169	90							-0.33	(-3.23)
86-80	140	67	5		0.85	(1.31)			-0.12	(-1.24)
86-79	169	103							-0.20	(-2.05)

Industry 383

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	87	48							-0.28	(-2.46)
85-80	71	29	2		0.03	(0.03)			-0.35	(-2.40)
86-81	64	23	2	1	0.68	(0.78)	...		-0.49	(-2.74)
85-79	87	48							-0.28	(-2.46)
86-80	71	32	2		-0.11	(-0.11)			-0.37	(-2.57)
86-79	87	51							-0.31	(-2.64)

Industry 390

diff.	n	nx	n80	n81	$\hat{\beta}_{80}$	(t ratio)	$\hat{\beta}_{81}$	(t ratio)	$\hat{\beta}_{TL}$	(t ratio)
84-79	77	42							-0.28	(-1.77)
85-80	66	29	2		0.12	(0.14)			-0.62	(-2.54)
86-81	63	28	1	0	...		...		-0.34	(-1.59)
85-79	77	42							-0.41	(-2.38)
86-80	66	33	2		...				-0.34	(-1.59)
86-79	77	45							-0.26	(-1.56)

Table 7: Absolute Increase in Estimated RTS when Mills Ratio is Included (Simple Fifth, Sixth and Seventh Difference Equations)

Industry

DIFF	312	313	321	322	324	331	332	342
84-79	0.0007	0.0650	0.0256	-0.0157	0.0051	0.0270	0.0046	0.0077
85-80	0.0134	-0.0152	0.0345	0.0000	-0.0279	0.0015	0.0010	0.0242
86-81	-0.0011	-0.0741	0.0046	-0.0000	-0.0146	0.0069	0.0045	0.0063
85-79	0.0172	0.1075	0.0447	-0.0082	0.0005	0.0164	0.0056	0.0352
86-80	0.0104	0.0060	0.0265	-0.0078	-0.0390	0.0050	-0.0001	0.0042
86-79	0.0086	0.0008	0.0449	-0.0138	0.0026	0.0460	0.0291	0.0033

DIFF	352	355	356	369	381	382	383	384
84-79	-0.0006	0.0212	-0.0432	0.0017	-0.0057	-0.1608	-0.0143	-0.1892
85-80	-0.0310	0.0392	0.0064	0.0321	-0.0042	0.0231	0.0653	-0.0722
86-81	-0.0341	-0.0418	-0.0121	-0.0771	0.0071	-0.0252	-0.0137	-0.0191
85-79	0.0211	-0.0043	-0.0472	0.2308	0.0001	-0.0232	0.0156	0.0765
86-80	-0.0339	-0.0123	0.0050	0.0252	-0.0126	-0.0239	0.0145	0.0015
86-79	0.0764	-0.0394	-0.1072	0.0509	-0.0054	-0.1139	-0.0046	-0.0468

Table 8: Iterative GMM Estimates by 3-digit Industry (Extended Data)

	7th Difference				6th and 7th Differences				5th, 6th and 7th Differences			
	N	α	β	RTS	N	α	β	RTS	N	α	β	RTS
312	538	.502	.407	.909	693	.516	.417	.934	822	.483	.482	.965
		(.072)	(.078)	(.083)		(.054)	(.067)	(.072)		(.047)	(.066)	(.069)
313	42	.719	.575	1.294	57	.489	.209	.698	65	.504	.197	.701
		(.263)	(.332)	(.486)		(.131)	(.203)	(.240)		(.067)	(.177)	(.160)
321	138	.511	.304	.815	171	.732	.161	.894	201	.673	.081	.844
		(.133)	(.132)	(.160)		(.096)	(.100)	(.120)		(.079)	(.081)	(.098)
322	100	.815	.219	1.034	132	.698	.244	.942	161	.623	.103	.727
		(.138)	(.160)	(.152)		(.101)	(.097)	(.125)		(.070)	(.053)	(.078)
324	48	.638	.396	1.033	63	.559	.404	.963	70	.756	.225	.981
		(.192)	(.109)	(.219)		(.093)	(.100)	(.119)		(.077)	(.071)	(.083)
331	91	.772	.119	.890	134	.414	.257	.671	157	.481	.227	.708
		(.240)	(.227)	(.382)		(.103)	(.160)	(.194)		(.092)	(.137)	(.168)
332	28	.398	.774	1.172	48	.628	.142	.770	55	.768	.010	.778
		(.407)	(.389)	(.210)		(.153)	(.124)	(.175)		(.115)	(.100)	(.134)
342	65	.566	.196	.762	96	.533	.303	.836	105	.514	.424	.938
		(.107)	(.119)	(.134)		(.080)	(.101)	(.104)		(.061)	(.097)	(.098)
352	74	.155	.254	.409	97	.258	.475	.733	105	.181	.541	.722
		(.138)	(.162)	(.202)		(.090)	(.122)	(.144)		(.071)	(.109)	(.119)
355	28	.343	.544	.887	33	.723	.225	.948	35	.047	.405	.452
		(.228)	(.293)	(.300)		(.112)	(.138)	(.153)		(.090)	(.110)	(.086)
356	43	.990	.018	1.008	58	.319	.208	.527	74	.349	.275	.623
		(.404)	(.256)	(.319)		(.197)	(.177)	(.165)		(.135)	(.140)	(.142)
369	30	.649	.417	1.066	39	.503	.194	.697	46	.481	.354	.835
		(.292)	(.214)	(.330)		(.122)	(.166)	(.216)		(.111)	(.147)	(.195)
381	127	.696	.470	1.166	177	.530	.222	.752	202	.419	.174	.593
		(.139)	(.147)	(.160)		(.102)	(.092)	(.122)		(.078)	(.090)	(.113)
382	25	-.086	.462	.376	47	-.078	.521	.443	58	.037	.654	.692
		(.378)	(.259)	(.308)		(.229)	(.203)	(.243)		(.145)	(.138)	(.164)
383	23	.513	.544	1.057	30	.486	.157	.643	31	.118	.215	.334
		(.174)	(.243)	(.302)		(.075)	(.113)	(.136)		(.057)	(.068)	(.073)
384	28	.870	.347	1.217	35	.520	.542	1.062	43	.946	.528	1.473
		(.188)	(.162)	(.136)		(.103)	(.097)	(.095)		(.092)	(.076)	(.083)

Table 9: Iterative GMM Estimates by 4-digit Industry (Extended Data)

	7^{th} Difference				6^{th} and 7^{th} Differences				5^{th}, 6^{th} and 7^{th} Differences			
	N	α	β	RTS	N	α	β	RTS	N	α	β	RTS
3111	43	.795	.580	1.375	62	.751	.547	1.298	70	.510	.631	1.140
		(.191)	(.099)	(.210)		(.139)	(.082)	(.150)		(.115)	(.088)	(.134)
3113	25	.906	-.674	.232	32	.634	.607	1.241	36	.968	.574	1.542
		(.213)	(.394)	(.344)		(.199)	(.199)	(.205)		(.083)	(.090)	(.104)
3115	20	.193	.490	.683	24	.398	-.469	-.071	27	-.216	-.448	-.665
		(.454)	(.413)	(.604)		(.169)	(.342)	(.344)		(.074)	(.149)	(.142)
3116	42	.182	.569	.750	55	.120	.019	.139	61	.263	.090	.353
		(.184)	(.247)	(.288)		(.139)	(.136)	(.169)		(.111)	(.116)	(.167)
3117	321	.775	.335	1.110	420	.658	.385	1.043	513	.662	.383	1.045
		(.088)	(.109)	(.106)		(.073)	(.092)	(.089)		(.062)	(.084)	(.084)
3132	22	.860	.131	.991	30	.784	.046	.831	34	.182	.207	.388
		(.157)	(.346)	(.256)		(.123)	(.116)	(.125)		(.054)	(.120)	(.103)
3211	65	.459	.263	.722	75	.516	-.019	.497	83	.512	-.072	.490
		(.175)	(.166)	(.245)		(.124)	(.127)	(.164)		(.097)	(.087)	(.135)
3213	44	.815	.563	1.378	61	.650	.425	1.074	78	.804	.347	1.151
		(.309)	(.257)	(.299)		(.149)	(.120)	(.172)		(.136)	(.101)	(.128)
3311	79	.694	.053	.747	115	.377	.200	.577	133	0.458	0.224	0.683
		(.317)	(.263)	(.500)		(.109)	(.168)	(.210)		(.098)	(.146)	(.181)
3522	28	-.041	-.530	-.571	35	.147	-.378	-.232	36	.124	-.326	-.201
		(.160)	(.229)	(.281)		(.055)	(.115)	(.130)		(.028)	(.075)	(.087)
3813	25	.699	.940	1.640	39	.792	.835	.626	46	.445	1.236	1.681
		(.137)	(.087)	(.132)		(.097)	(.070)	(.107)		(.076)	(.067)	(.087)
3819	28	.662	.297	.959	47	.695	.497	1.193	55	.754	.295	1.049
		(.663)	(.204)	(.543)		(.232)	(.126)	(.220)		(.134)	(.089)	(.144)
3843	23	1.288	-.287	1.001	26	.297	.634	.931	35	-.085	.912	.826
		(.217)	(.316)	(.229)		(.062)	(.092)	(.073)		(.067)	(.084)	(.059)

Table 10: 3-digit and 4-digit Industry Rankings by RTS and β

Iterative GMM - Seventh Difference Equations

Industries Ranked by Estimated RTS			Industries Ranked by Estimated β		
3-digit Industry	RTS	Suspect?°	3-digit Industry	β	Suspect?°
313 (beverages)	1.294	yes	332 (furniture)	.774	
384 (transport equip)	1.217		313 (beverages)	.575	yes
332 (furniture)	1.172		355 (rubber)	.544	
381 (metal products)	1.166		383 (electric mach.)	.544	
369 (non-metallic min)	1.066		381 (metal products)	.470	
383 (electric machinery)	1.057		382 (non-electric mach.)	.462	
322 (clothes)	1.034		369 (non-metallic min.)	.417	
324 (shoes)	1.033		312 (food)	.407	
356 (plastics)	1.008		324 (shoes)	.396	
312 (food)	.909		384 (transport equip.)	.347	
331 (wood products)	.890	yes	321 (textiles)	.304	
355 (rubber)	.887		352 (misc. chemicals)	.254	yes
321 (textiles)	.815		322 (clothes)	.219	
342 (printing)	.762	yes	342 (printing)	.196	yes
352 (misc. chemicals)	.409	yes	331 (wood products)	.119	yes
382 (non-electric mach.)	.376		356 (plastics)	.018	

4-digit Industry	RTS	Suspect?°	4-digit Industry	β	Suspect?°
3813 (structural metal)	1.640		3813 (structural metal)	.940	
3213 (knitting)	1.378		3111 (meatpacking)	.580	
3111 (meatpacking)	1.375		3116 (grain mills)	.569	
3117 (bakeries)	1.110		3213 (knitting)	.563	
3843 (autos)	1.001		3117 (bakeries)	.335	
3132 (wineries)	.991		3819 (misc. metal prod.)	.297	
3819 (misc. metal prod.)	.959		3211 (spinning/weaving)	.263	
3116 (grain mills)	.750		3132 (wineries)	.131	
3311 (sawmills)	.747	yes	3311 (sawmills)	.053	yes
3211 (spin/weaving)	.722		3843 (autos)	-.287	
3113 (fruit/veg. can.)	.232		3522 (pharmaceuticals)	-.279	yes
3522 (pharmaceuticals)	-.571	yes	3113 (fruit/veg. canning)	-.674	

° Industries which shrank more than forty percent in real terms over the sample period 1979-1986 are considered "suspect".

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